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The Cryosphere Discussions

Interactive comment on "Greenland ice sheet surface mass balance: evaluating simulations and making projections with regional climate models" *by* J. G. L. Rae et al.

J. G. L. Rae et al.

jamie.rae@metoffice.gov.uk

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We thank the reviewer for his/her comments, and provide our responses below.

COMMENT: *On p. 2061, the authors say that an aim of the paper is to provide GrIS surface mass balance projections "with greater accuracy than is possible from coarser-resolution general circulation models." On p. 2062, they state that GCMs "generally have insufficient resolution to represent the orography at the margins of the ice sheets." On p. 2081, they say that "RCMs can resolve the steep topography at the margin of the ice-sheet better than GCMs, and thus produce a more realistic simulation." These statements reflect the conventional wisdom that high grid resolution is necessary for*

a realistic simulation of Greenland's SMB. The RCM results, however, suggest that surface physics is critical, without showing that grid resolution is equally important. For example, the MAR, HIRHAM5, and HadRMP3 models have similar grid resolution, but very different SMB simulations. I suggest that the authors qualify these statements, citing the manuscripts submitted recently (after the Rae et al. paper was written) by Vizcaino, Lipscomb, and co-authors to the Journal of Climate. This work shows that the Greenland SMB simulated by a global climate model (the Community Earth System Model, CESM) compares well with that of RACMO. CESM has sophisticated snow physics, along with a sub-grid tiling scheme for downscaling atmospheric forcing to different elevation ranges in each grid cell. The success of this approach is consistent with the findings of Rae et al. on the importance of surface physics, while suggesting that lower grid resolution can be compensated to some degree by sub-grid tiling.

RESPONSE: We thank the reviewer for raising this important point. While we are reluctant to cite papers that have not yet been published, we are happy to address this point by making the following changes to the manuscript:

- On page 2062, where we say: "[GCMs] generally have insufficient resolution to represent the orography at the margins of the ice sheets...", we have now added the qualifier "...unless they use subgrid-scale tiling to represent the range of elevations present in each gridbox".
- The sentence on page 2081 (in the conclusions section) to which the reviewer refers originally read: "RCMs can resolve the steep topography at the margin of the ice-sheet better than GCMs, and thus produce a more realistic SMB simulation, which also depends critically on the use of satisfactory schemes for ice-sheet surface energy and mass balance". We have now changed this to: "We have shown that a realistic SMB simulation depends critically on the use of satisfactory schemes for ice-sheet surface energy and mass balance. In addition, it is important to resolve accurately the steep topography at the ice-sheet mar-

gins. In the present study, we have achieved this by using RCMs, which have higher resolution than GCMs; an alternative approach would be to run GCMs with subgrid-scale tiling schemes to represent the range of elevations present in each gridbox".

We hope that these changes make it clear that (as the reviewer correctly points out) it is not the resolution per se that is important, but rather the representation of the steep topography near the margins of the ice sheet, which can be achieved either by running higher-resolution models, or by using subgrid-scale tiling.

COMMENT: *On p. 2080, and again on p. 2082, the authors cite the ranges found by Gregory and Huybrechts (2006) and Robinson et al. (2012) for the temperature at which Greenland's total SMB becomes negative. They simply state that the various MAR, HIRHAM3, and HadRM3P results lie at the edge of or outside these ranges. It would be helpful they could assess whether the new RCM results (for MAR in particular) are more credible than the previously published ranges, and if so why.*

RESPONSE: This is an interesting question, but unfortunately one that is difficult to answer, as the studies of Gregory Huybrechts (2006) and Robinson (2012) did not include evaluation of model output against observations. However, we would point out that in our evaluation against observations (Section 3), MAR performed better than HadRM3P and HIRHAM5. In general, it is unwise to assume that accurate representation of the past automatically implies accurate representation of the future, but in this case we have attributed the better performance of MAR relative to HadRM3P and HIRHAM5 to a better representation of key physical processes, so this assumption is probably valid here. We can therefore reasonably conclude that the temperature threshold for negative SMB obtained from MAR is probably more credible than those obtained from HadRM3P and HIRHAM5. In addition, the temperature threshold from MAR lies within the ranges of both of the previous studies cited, while those from HadRM3P and HIRHAM5 lie outside these ranges. We can therefore conclude that the

temperature threshold from the most credible model in our study (MAR) is consistent with, but not more or less credible than, the estimated ranges from the two studies cited. The temperature threshold from HadRM3P is much higher than the ranges in those studies, and is almost certainly not credible, as the temperature sensitivity of HadRM3P is probably far too low, as discussed elsewhere in the manuscript. The temperature threshold for HIRHAM5, which is probably not as credible as that for MAR, is slightly lower than the previously-published ranges.

We have therefore made the following changes:

- In Section 4.5, after the sentence: "Similarly, the value for HadRM3 is higher, and that for HIRHAM5 lower, than the range of 2.0–3.5°C found by Robinson et al. (2012),while the value for MAR lies towards the lower end of that range", we have added: "MAR, which has the most detailed surface scheme and was the best-performing of these three models in the evaluation against observations (Section 3), is therefore consistent with these previously-published estimates". This is followed by the existing sentences, which already explain why the thresholds for HadRM3P and HIRHAM5 lie outside these ranges.

- In the conclusions section, we have changed: "The equivalent thresholds for annual-mean Tas change are similar to those for JJA, and, when taken relative to the pre-industrial period, the thresholds for HadRM3P and HIRHAM5 are outside the range of 3.2–6.5°C found by Gregory Huybrechts (2006) via pattern-scaling, while that for MAR is at the extreme lower end of this range. Robinson et al. (2012) performed RCM simulations with perturbed parameters, and obtained a range of 2.0–3.5°C for this threshold. The thresholds obtained with HadRM3P and HIRHAM5 are both outside this range, while that obtained with MAR is at the lower end" to: "The equivalent thresholds for annual-mean Tas change are similar to those for JJA, and, when taken relative to the pre-industrial period, the threshold for MAR is consistent with previous estimates (Gregory Huybrechts 2006;

Robinson et al. 2012), while those for HadRM3P and HIRHAM5 are outside the ranges given in those studies". The reasons why the estimates from the three RCMs differ are summarised earlier in the paragraph, so we have not re-stated them in this sentence.

COMMENT: *As stated in Table 1 and elsewhere, RACMO was not used for simulations of the 21st century. Given that it has arguably the best SMB simulation for the recent past, I am curious to know why it was not used for the 21st century projections, and whether there are plans to use RACMO for such projections.*

RESPONSE: This was a matter of resources. This work formed part of the ice2sea project, in which some groups ran their RCMs for Greenland, and some for Antarctica. The decision was taken that the Utrecht group, who run RACMO, would focus their resources on Antarctica rather than Greenland. They are now performing 21st century simulations for Greenland, driven by boundary conditions from CMIP5 GCMs for RCP scenarios, and the group intends to submit a paper on these simulations in the near future. Some preliminary results are presented in Table 3 and Fig. 5 of Fettweis, X., et al. (2012, The Cryosphere Discuss., 6, 3101-3147.

COMMENT: *Figure 2 is hard to read and should be reformatted.*

RESPONSE: We agree that the plots in Fig. 2 are too small and difficult to read. After experimenting with various orientations, we have decided that the best solution is to split the figure across two pages. This allows the size of the legend (and the font in it) in Fig. 2a to be increased. We have also reproduced the legend from Fig. 2a in Fig. 2e on the second page.

COMMENT: *Some figures should be renumbered to be consistent with the order cited in the text. In the submitted manuscript, for example, Fig. 3 is mentioned (p. 2067) before Fig. 2 (p. 2068).*

RESPONSE: We have now renumbered the figures so that they are consistent

with the order in which they are cited in the text.

COMMENT: *On p. 2069, l. 29, "was" should be "were".*

RESPONSE: Now corrected.

COMMENT: *On p. 2082, l. 23, the word "are" is missing.*

RESPONSE: Now corrected.

Interactive comment on The Cryosphere Discuss., 6, 2059, 2012.